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COMMUNICATIONS

COLOUR BLIND FEMALES

THE INHERITANCE OF COLOUR BLINDNESS IN MAN *(concluded)*

BY

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AMONGST the comprehensive literature concerning colour blindness the works of Dalton, Horner, and Nettleship, are some of the most valuable. Even before the time of Dalton, however, the hereditary character of the anomaly was touched upon, so that one nearly agrees with Magnus's statement in 1877 : " The hereditariness of colour blindness has been known as long as the defect itself. All who have seen many colour blind, will have often noticed that it was in their mother's family that other cases were found." The occurrence in families was already mentioned in the very first case of colour blindness described (Joseph Priestley in 1777), from which we learn that the shoemaker, Harris, had two colour blind brothers. When describing his own anomaly, Dalton wrote also of his two colour blind brothers, mentioning a total number in his work of 40 colour blind men (1798).

In his small but very important treatise, "Experiments on Colour," Lord Rayleigh first describes his apparatus by means of which he can obtain comparatively pure colours, in a manner enabling him to compare a pure yellow colour with another yellow colour obtained by a mixture of red and green (Rayleigh's equation). Thereafter he states that a red-green mixture, which to himself and most beholders appeared like a pure yellow, corresponding to the D line in the spectrum, appeared to his three brothers-in-law Balfour as red, "almost as red as sealing wax." In order to suit the colour mixture to their eyes the red part had to be made so weak that the mixture appeared definitely green to normal eyes. The three brothers-in-law agreed mutually in their colour perception, and they all accepted the identical equation (the deuteranomalous equation). A fourth brother-in-law and three sisters-in-law had normal colour perception (1881).

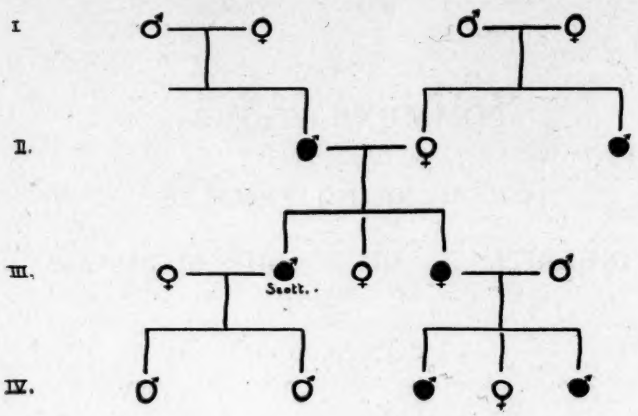

FIG. I.

Family Scott.

We possess from the 19th century and the last twenty years of the 18th century a number of papers and pedigrees referring to colour blindness. It will be shown below how the anomaly in these tables, perfectly and without exception, falls into line with the interpretation of *colour blindness as a recessive sex-linked character.* This aim has necessitated a careful examination of the original pedigrees, as faulty and incorrect extracts from some of them have been quoted again and again in text-books and periodicals as exceptions from the rule, which in reality is unbroken.

The first, and even now one of the best pedigrees, was published by Lort in 1778, based on a letter from Scott, in which he describes his own condition and that of his near relatives. Scott states:

"My business was behind a counter many years, when I had to do with variety of colours. I often, when alone, met with difficulty, but I commonly had a servant in my way to attend me, who made up my deficiency. I have been now several years from trade. My eyes, thank God, are very good at discerning men and things." And further: "My mother's one brother had the like impediment with me, though my mother knew all colours very well."

In spite of the few persons included in this pedigree, it really shows most of the facts needed; hence it will be given here.

We must assume that Scott had inherited the anomaly from his mother, who most probably was a conductor, as she had a colour blind brother and got a colour blind son.

Thus the fact that one of Scott's sisters was colour blind is explained naturally enough, as she may have inherited the defect both from her father and her mother. The sons of this colour blind sister were all as a matter of course, colour blind, while Scott's own children were colour efficient. All these facts are now naturally and easily explained by the laws.

Scott's sister is the first colour blind woman ever described. It is true, that as early as 1683, Tuberville published his extraordinary case: "A maid, 22 years old, came to me from Banbury who could see very well, but no colour besides black and white. She had such scintillations by night (with the appearances of bulls, bears, etc.) as terrified her very much. She could see to read sometimes in the greatest darkness for almost a quarter of an hour."

Usher seems to be correct when saying, with regard to this case that "it was probably total colour blindness with day blindness or possibly some hysterical condition."

The brief papers of Harwey, J. Butters, Nicoll, Sommer, and Rozier, may just only be mentioned in passing on to the case of James Milne, published in 1825 by Combe, who describes colour blindness in J. Milne, his two brothers, his mother's father, and one male cousin. This good pedigree was continued by Usher in 1912, showing how the anomaly adheres to the typical rule throughout seven generations.

The hitherto most inexplicable pedigree of colour blindness was published in 1838 by F. Cunier, who himself admits that it varies in a very marked degree from all similar tables published previously. In this case the anomaly is supposed to have been passed on from mother to daughter in five successive generations, attacking all females (12) while all males (8) went free. Cunier states, that while examining a married lady of 58, he learnt that she was colour

blind, inasmuch as she never saw any difference between dark blue and cherry red colours. He was allowed personally to test her daughters and her daughter's daughters by means of skeins from a work basket. Further, he learnt that the maternal grandmother, mother, and the only aunt of his patient had suffered from the same inability, while all male members of the family had a good colour sense.

The anomaly described by Cunier cannot, however, have been the hereditary red-green blindness, for a closer study of his original paper reveals the following description of the pathological findings in their eyes, as most pronounced in the eldest daughter and daughter's daughter of his patient. "The pupil has a normal shape and reacts normally, but has not the usual black colour; in the depths of the eye (*'au fond de l'oeil'*), slightly above the horizontal meridian and outwards, an oval yellowish spot is seen, with a green dent in the centre." These facts he interprets as indicating *l'absence du pigment noir de la chorioïde.*

"These women did not endure bright sunlight, especially when coming out in the sun from a dark room, or when the sun suddenly broke through a cloud."

The male members of the family had quite normal eyes, and showed neither colour blindness nor photophobia. It was stated that no disease of the eyes nor any case of blindness was known in the family, and this fact Cunier thought remarkable, because until that time he had found defective sense of colour only *"dans l'amaurose ou dans la cataracte."* Cunier naturally did not possess any ophthalmoscope and was therefore not capable of making any exact diagnosis. We are, however, justified in assuming the alterations in the eyes to be the cause both of photophobia and imperfect sight as well as of their defective colour sense. Herbert Parsons is undoubtedly right when stating in his "Pathology of the Eye": "Cunier published a remarkable family, in which through five generations only women were affected, but the case is almost certainly not one of congenital colour blindness."

In 1840, Szokalski's paper on colour sense was published, containing a chapter on colour blindness, giving, however, no fresh pedigrees. Concerning Cunier's information he says: *"une observation très curieuse, qui est dans une contradiction flagrante avec toutes celles qui ont été recueillies, jusqu'à ce moment."*

Dr. Pliny Earle of Philadelphia in 1845 described his own colour weakness, adding a very comprehensive pedigree, that is unusually clear: Out of 3 colour blind brothers 1 has 5 normal daughters, who together have 11 colour blind sons, and 1 normal daughter's daughter, who has 1 colour blind son. The pedigree has been carried forward in an excellent manner by Nettleship and Usher and was published in 1915.

Holmgren rendered great services by his extensive research work concerning the occurrence of colour blindness in Sweden, and he also was deeply interested in the problem of its hereditary nature, which he has dealt with in several publications. He has, however, never published any pedigrees.

The usual hereditary type of colour blindness was first demonstrated and described by the Swiss ophthalmologist Horner, later affirmed by Burckhardt and Bollinger. Horner in his very significant work "Hereditariness of Daltonism," gives 2 uncommonly fine pedigrees of red-green blindness in 6 and 4 generations respectively, and on the basis of these two he formulated his well known law, viz.: "that sons of daughters whose fathers were colour blind are most likely to be the same, although not without exception; or colour blindness is transmitted in the reversible type from grandfather to grandchild."

In most references to Schoeler's instructive pedigree there occurs an inexactitude, which causes it to appear inexplicable by the laws concerning a recessive sex-linked character, although the pedigree *per se* is unusually fine and typical.

The original table of Schoeler is this:

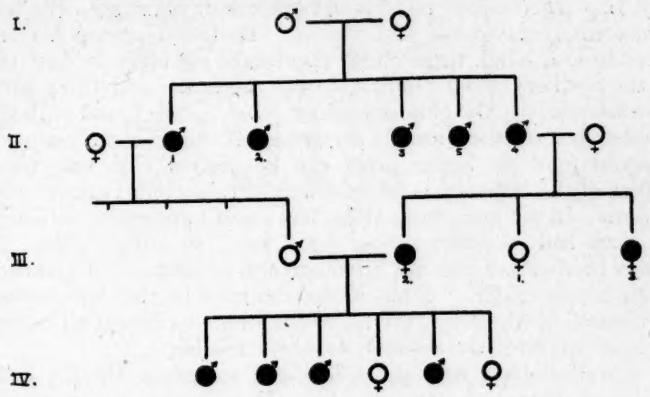

FIG. II.
Schoeler's pedigree.

A married couple had 5 sons, all colour blind, which fact might indicate that the mother was colour blind, only no statement is made in this respect. One of these sons had 3 daughters, of which 2 were colour blind, which proves that their mother must have been a conductor. And 1 of the 2 colour blind sisters (in a marriage with her colour efficient cousin) had only colour blind sons and two normal daughters.

In the inexact quotation* of this table the six sisters and brothers of the fourth generation, are wrongly said to be children of No. II_5 with no fourth generation and the occurrence of the anomaly remains quite inexplicable.

Faulty explanations of the pedigrees showing colour blindness are often brought forth by various authors, who thus procure examples of anomalies that seemingly do not fall in with the laws. Thus, several authors have mentioned a pedigree published by Stilling explaining the colour blindness of two brothers as an inheritance from the grandfather through the unaffected father, while the anomaly is explained in a much more natural way as inherited from the mother (a conductor), the wife of the unaffected father.

Further, Sir William Abney in his book on colour vision mentions several cases where, in his opinion, the anomaly has been transferred from father to sons, and he even supposes Dalton's father to have been colour blind. Even if that was the case, we should conclude from what has been said above, that Dalton did not inherit his anomaly from the father, but from the mother, who was probably a conductor.

A series of investigations undertaken by Cohn in 1878 shows very clearly that when colour blind men have colour blind sons the fact does not imply any cause and effect. "He found among his one hundred colour blind, three times the father red blind, in fourteen cases the brothers colour blind, and once the father and three sons. In no case were all the children colour blind." As found with the incomplete tests of the time the percentage of colour blindness being just about four, no better proof can be desired that the colour blindness of the fathers has no influence on the colour perception of their sons. In the same work Cohn has a small pedigree: A colour blind man had a sister, whose sons were all colour blind; his mother's brother had a colour blind son and a colour blind grandson through his daughter. Even if the mother's brother were colour blind himself, of which we have no information, his son, at all events, must have inherited the anomaly from the mother.

As is well known, Albrecht Nagel was red-green blind, and he has himself described his anomaly. His pedigree shows that a colour blind father and a colour efficient mother (who must have been a conductor) had two sons of whom one was colour blind, and three daughters of whom two were colour blind. This pedigree is referred to in L. Plate's great text-book as the only known example of red-green blindness occurring as a dominant character, and Plate adds: "The rule, as is well known, being that the red-green blindness is a dominant sex-limited anomaly (dominant in ♂ recessive in ♀)!"

R. C. Punnet makes partial use of the same terms in his book,

* Nagel's *Jahresbericht*, 1878, p. 117.

"Mendelism;" but in these terms we cannot agree. Also he states, that "since neither colour blind men nor heterozygous women (conductors) are common, the chances of a marriage between them are very rare." Above it has, however, been proved that there is every reason to believe that such marriages are not so rare—1·8 per cent.

The best investigations concerning the hereditary character of eye-defects and anomalies were those carried out by E. Nettleship, the late lamented ophthalmologist. Most of his pedigrees deal with the hereditary eye-defects and are found in his admirable Bowman lecture, "On some Hereditary Diseases of the Eye";* as, however, hereditary colour blindness must be considered an anomaly

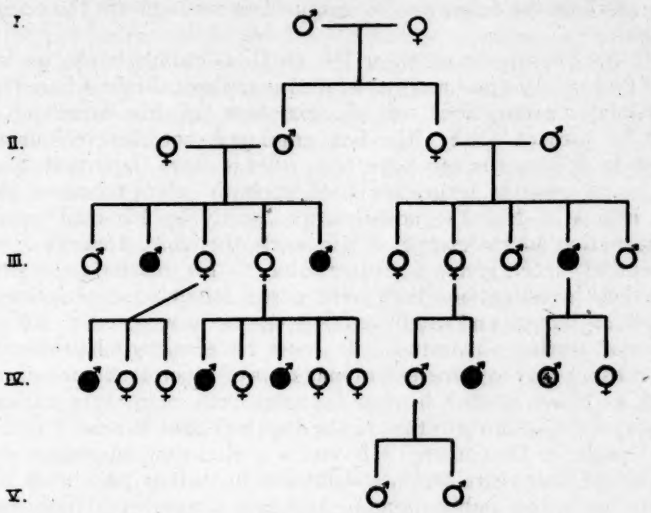

FIG. III.

Rivers's pedigree from the Todas.

more than a disease, he has not dealt with this subject in the work cited. All the same, his industry produced 10 pedigrees of colour blindness,† of which about half are almost completely worked out. After his death, C. H. Usher arranged and published further four pedigrees, of which one is a continuation of Pliny Earle's excellent pedigree from 1845.

Nettleship, also, was much interested in colour blindness in women, and he succeeded in examining or obtaining information of about 20

* *Trans. of the Ophthal. Soc.*, Vol. XXIX.
† *Trans. of the Ophthal. Soc.*, Vols. XXII, XXIII, XXVI, XXVIII, XXXII.

such cases. Three times he found the anomaly in one of twins. Twice the twins were girls, and at any rate one couple was proved to be mono-oval. They were as like as two drops of water; but one was colour blind, and remarkably enough, the other was found to be colour efficient. They were not, however, more than 8½ years old at the last examination. (A further examination with our modern tests of these twins and of their issue would be of very great interest.) Once the twins were of different sex, and, of course, the boy was the one colour blind.

As a curiosity, I give below a pedigree from W. H. R. Rivers's investigations among the Todas mentioned above, where the anomaly occurs in two families related to each other, but only in one of them the defect can be imagined as coming from the common origin.

It is necessary to mention Dr. G. F. Göthlin's work, he being the first to elaborate amply detailed genealogical tables based upon personal examinations of all members of his families, who are at present alive. He has employed the best colour sense tests in existence in our time, and, what is more important, he has as far as possible *invariably* used spectral colour mixtures, partly by means of Nagel's anomaloscope, partly by his own portable polarisation anomaloscope. His work, therefore, presents a more scientific aspect, giving a greater value to his results than all the previous investigations that were partly based on descriptions by word of mouth and traditions, partly on tests carried out with inferior testing apparatus. It must be admitted, however, that Nettleship also to some extent made use of spectral colour mixtures, and, as above quoted, he has brought forth many very admirable pedigrees. Göthlin's lecture in the Psychological Research Institute in Upsala, in December, 1915, was a preliminary summary of the results of four years' work, and interesting things may be expected from his further publications. Amongst a number of families he has selected eight, whose pedigrees he publishes in his treatise; the pedigrees are not very extensive, but so good and so fully detailed that he must be considered justified in drawing his important conclusions.

Thus, for instance, he appears to have proved Holmgren's and Abney's supposition, viz.: that "the nature of the colour blindness within the same family is always the same," to be not always reliable. On the basis of the previous statement, we cannot, however, agree with Göthlin, when he concludes: "Personally, I am of the opinion that when colour blindness is very marked in the father, the inheritance may be manifest even in women who are heterozygots." He believes that he has seen two such cases, but in both the family of the mother has hardly been examined; it is true that in one case the examination included the mother's

brother, sister, and sister's daughter, but that is insufficient to prove anything. Göthlin finishes his conclusions as follows:

"The result of this research among families proves that there exists between a number of colour sense types a genetic connection which has not previously been demonstrated. A genetic connection has thus been discovered between protanopia and deuteranopia, and also between deuteranopia and deuteranomalia, probably also between protanopia and protanomalia." In his paper he goes into the problem more deeply, expressing partly the same views which Köllner just as strikingly has set forth and which, if they prove to be correct, will greatly simplify our conception of the various forms of anomalies of colour sense. Köllner states: "The Rayleigh equation of the deuteranomalous may be changed into the equation of the protanomalous by the simple process of imitating, by means of a suitable absorbing medium, the conditions of light under which the protanopes and the protanomalous distinguish themselves from the deuteranopes and the deuteranomalous. In spite of the varying Rayleigh equations we may therefore suppose that the three colours in question (red, green, yellow) may have approximately the same colour value for the protanomalous as for the deuteranomalous. Further, we may imagine that the difference between the Rayleigh equations of the two forms may be dependent in the first place on varying values of light."

In my own investigations of colour blind families, I have especially aimed at finding the highest possible number of red-green blind women, because in their families one would presumably find numerous red-green blind persons. Experience has proved the assumption to be right, since precisely by seeking colour blind women I have found families most decidedly suited for further investigations.

In the course of the last two years and aided by kind colleagues* I have had the opportunity of examining very closely 16 colour blind women (dichromatics and anomalous trichromatics), their families, and nearest relations. Together with the above-mentioned 20 school girls, this will make 36 cases of colour blind women. Later on I hope to publish some of my pedigrees, which *all* show how the anomaly is inherited in the typical manner; whenever I have been able to ascertain the facts, these colour blind women had colour blind fathers and colour blind sons.

Conclusion: The results obtained by a search through medical literature and through all pedigrees published with regard to red-green blindness as an hereditary character, will thus be seen to conform completely with the results of my own investigations, proving that the congenital red-green blindness invariably and

*I beg to express my special gratitude to the ophthalmologist Dr. Harald Gjessing, Drammen.

without exception follows the laws of inheritance as a ***recessive sex-linked character.*** The few pedigrees which have previously been quoted as exceptions from the ruling principles, have on a closer examination of the original papers proved to be partly misquoted and partly to be concerned with anomalies other than the congenital red-green blindness. No single exception can be demonstrated to the unyielding principles.

It is true, that Bateson in his book, "Mendel's Principles of Heredity," 1913, mentions quite casually a case of an alleged colour blind mother with an alleged colour efficient son. The case has been brought to his notice by Miss J. E. Downey, of Wyoming University, but it cannot be fully accepted until all details of the case have been investigated. Among other things it must be excluded that the mother's colour blindness is acquired; also it is necessary to know what sort of colour tests were employed.

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DR. EDRIDGE-GREEN'S THEORIES OF VISION

(*concluded*)

BY

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VIII. The Evolution of the Colour Sense

Dr. Edridge-Green's views on the Evolution of the Colour Sense are based on the following considerations:—(1) Psycho-physical theory; (2) Colour vision in primitive races (*Trans. Ophth. Soc.*, Vol. XXI, 1901, p. 182); (3) The order of disappearance of colours in the spectrum as the intensity is gradually diminished (Edridge-Green's "Physiology of Vision," p. 169); (4) The atavistic theory of colour blindness.

(1) *The Psycho-physical Theory.*—"The sense of sight must have been first developed for those waves which produced their maximum effect upon the sensitive protoplasm" (p. 213). "The wave-length of the physical stimulus is the physical basis of the sensation of colour" (p. 214). "The eye would first discriminate between the rays which are physically most different—the red and violet, that is presuming that the eye had become sensitive to this range" (p. 214). In this stage "the luminosity curve should be the same as the normal sighted" (p. 216). "The next stage in the evolution of the colour sense was when a third colour appeared at the third point of physiological difference, that is, in the centre of the spectrum in the position of the green" (p. 216). "As evolution proceeded the power of differentiation occurred in the regions between the red and the green and the green and the violet until a stage was reached in which a fourth colour, yellow, was seen at the next point of greatest physiological difference" (p. 217). "The next step in the process of evolution occurred when the retino-cerebral apparatus was able to differentiate a fresh colour between

the green and the violet, namely, blue, five definite colours being seen in the spectrum" (p. 217).

This theory of the evolution of the colour sense appears to me to be so extremely speculative as to be of little practical value. In the first place it would apply equally to the auditory sense, and it is well known that all attempts to bring the theory of colour vision into line with that of hearing have failed (Cf. Helmholtz, "Physiologische Optik," 3rd Ed., Vol. II, p. 97).

Second, the "waves which produced their maximum effect upon the sensitive protoplasm" are unknown, for it is obviously a specific stimulus, and if any conjecture is made on this basis the most probable waves would be in the yellow, which are of maximum luminosity.

Third, presuming the sequence was as described, why did yellow precede blue, and why do not orange and indigo appear simultaneously.

Fourth, there is good evidence that the Weber-Fechner law is valid over the ordinary range of visual sensations, though it fails for very high and very low intensities. If it was valid in the early stages of evolution, then the sensations should increase in arithmetical proportion as the stimuli increase in geometrical proportion, or in other words the sensation would vary with the *logarithm* of the stimulus. The results on this basis would differ according as the wave-length or the brightness were taken as the criterion. These, however, are pure speculations, and throw little, if any, light on the evolution of colour sense.

Almost simultaneously with Dr. Edridge-Green's first paper on the Evolution of the Colour Sense, McDougall (*Mind*, Vol. X, N.S., No. 38, p. 212; quoted in my book, pp. 282-4), dealt with the same subject, also on the basis of a psycho-physical theory. Like Dr. Edridge-Green, he thinks that two colours at the ends of the spectrum were first perceived. He thinks, however, that these were yellow and blue. At a later stage differentiation again took place in the yellow, giving rise to red and green. McDougall is a strong supporter of Young's theory, supplemented by the duplicity theory. On these theories any further differentiation is unnecessary to explain the facts.

Dr. Edridge-Green is strongly of opinion that red and violet were the first colours seen, and that dichromics see only red and violet (pp. 192-195). It is certain, however, that extreme cases of colour-blindness, if they limit themselves to two colour names only, which is uncommon, almost invariably speak of blue and yellow. Even normal people generally describe the whole of the blue-violet end of the spectrum as "blue." It is highly improbable that people would call one of the earliest developed colours violet, since this colour is almost unknown in nature. Moreover, there is a considerable

amount of other evidence in favour of blue and yellow as opposed to red and violet.

(2) *Colour Vision in Primitive Races.*—This evidence is derived from two sources—philological and anthropological. I have reviewed the literature in my "Introduction to the Study of Colour Vision" (pp. 145, *seq.*). As evidence of evolution the results are probably valueless. It is unlikely that any material change in the evolution of the colour sense has occurred within historical times. That evolution in colour terminology has occurred is undoubted, just as the finer shades of meaning of all kinds are progressively reflected in the vocabularies of advancing peoples. Even, however, at the present time, ordinary people, and often educated people, call a greenish blue either green or blue indiscriminately, and orange red or yellow. Of course, this indefiniteness is largely due to carelessness, but it is also fostered by varieties of illumination. Very little importance from the point of view of colour vision is to be attached to crudities of this kind.

I have also collected the comparative (phylogenetic) and ontogenetic evidence on the subject (*loc. cit.*, pp. 130 *seq.*, 152 *seq.*). In both cases the evidence is inconclusive. That relating to the colour vision of animals is very contradictory, and in any case does not support Dr. Edridge-Green's theory. That relating to the development of the colour sense in the child is valueless for the purpose under consideration.

(3) *The Order of Disappearance of Colours in the Spectrum as the intensity is gradually diminished.*—Dr. Edridge-Green says that they disappear "one by one in the reverse order of the development." This statement is not in accordance with the observations of chromatic thresholds, by Charpentier and others, particularly the very accurate investigations of Abney and Festing, and Abney and Watson. Burch has proved the variability of these phenomena according to the condition of the retina owing to previous stimulation.

(4) *The Atavistic Theory of Colour Blindness.*—These speculations lend little support to any theory of colour vision. Since, however, the Young-Helmholtz theory is directly based upon the fundamental assumption which Dr. Edridge-Green makes, viz., the interpretation of colour sensations in terms of physical stimuli, it is not improbable, *on that assumption*, that colour defects interpreted in terms of the Young-Helmholtz theory afford some evidence as to the evolution of the colour sense, for it is not improbable that a congenital defect of this nature is atavistic.

IX. Simultaneous Contrast, After-Images, and Successive Contrast

The phenomena of After-Images and Colour Contrast are well

known to have been difficult of explanation by Young's theory, and the efforts of Helmholtz to account for them have never been regarded as satisfactory. Hering's theory offered more plausible explanations, but failed to account for more phenomena of colour vision than it explained. In 1901, McDougall (*Mind*, Vol. X, N.S., pp. 52, 210, 347), published three papers in which he showed that the facts of after-images and so-called "induction" could be fully explained in consonance with Young's theory. Probably the scant attention which McDougall's exhaustive experiments have received is due to the medium of publication. McDougall's explanation of induction is expressed in terms of Young's theory (see my book, pp. 279-280), which somewhat obscures the fact that it is identical with that adopted by Dr. Edridge-Green, though the latter does not acknowledge the source of the theory. McDougall himself points out that the principle was indeed set forth in 1867 by Rollet. It has generally been held that the complementary (contrast) colour seen in simultaneous contrast experiments is a purely subjective phenomenon. The Rollet-McDougall explanation is based upon the fact that the white light by which the contrast surface is seen contains light of all wave-lengths, and therefore includes those wave-lengths which give rise to the "induced" colour. Hence the contrast colour is present objectively, and requires no purely psychological explanation. Dr. Edridge-Green also adopts McDougall's explanation of the colour "induced" upon a grey surface, viz., that it is due to the inhibition or subtraction of the inducing colour from the grey.

McDougall's experiments on these peculiarly variable and elusive phenomena of vision cannot be disregarded. His final conclusion may be quoted. "Young's theory," he writes, "if we incorporate with it the hypothesis of the separate white-exciting apparatus having its retinal seat in the rods, as urged by von Kries, Rollet's theory of contrast and the theories of induction and after-images set forth above, explains fully all the phenomena described above; and, with the exception of certain observations of a complex character, very difficult to interpret, such as Müller's experiments on galvanic stimulation of the eyes and some of Professor Sherrington's flicker experiments, I cannot discover any important fact connected with light and colour-vision with which it is incompatible or that it fails to illumine."

With regard to successive contrast Dr. Edridge-Green still lays great stress upon his experiments with Professor A. W. Porter (*Proc. Roy. Soc.*, B., Vol. LXXXV, 1912, p. 434) in spite of the fact that their fallacies have been exposed by Burch (*Ophthalmoscope*, Vol. XI, 1913, p. 143). In his "Physiology of Vision" Dr. Edridge-Green says: "The greatest precautions were taken to prevent the admixture of stray light in those experiments which were of a

crucial character"; yet in the original paper the authors say: "In a dark room, in which, however, there was a certain amount of stray light." Burch says: "The phenomena recorded can all be explained when the stray light is taken into account, and they agree perfectly with Young's theory"; and "I have described in my paper 'On the relations of Artificial Colour Blindness to Successive Contrast' (*Proc. Roy. Soc.*, Vol. LXVI, 1900, p. 206) various methods of observing the phenomena of successive contrast with really pure spectral colours, using stimuli no stronger than those employed by Professor Porter and Dr. Edridge-Green. My results are different from theirs, and are in all respects quite in accordance with the theory expounded by Thomas Young."

X. Varieties of Colour Blindness

It has already been mentioned that Dr. Edridge-Green divides cases of colour blindness into two classes, according as the defect is one of light perception, or one of colour perception or differentiation without any defect in light perception; both defects may be present in the same individual. He cites five varieties of defective light perception (*vide supra*, p. 367), and classifies the varieties of defective colour perception according to the colour names employed (*vide supra*, p. 360). Even of a single one of his groups of colour blind, Dr. Edridge-Green says (*Ophthalmoscope*, Vol. XII, 1914, pp. 77, 79):—"There may be shortening of the red or the violet end of the spectrum; there may be defective perception for some of the other spectral rays; the luminosity curve may have its maximum at a different place from the normal; there may be defective perception when the image on the retina is diminished in size; and the size of the neutral region is very variable." "(1) There are many degrees and varieties of dichromic vision. (2) There are not two well-defined varieties of dichromic vision, there are innumerable gradations connecting the two. (3) In many cases precisely the same errors are made both by those with and those without defective perception of red, when the rays for which there is defective perception are not involved. (4) All dichromics are not equally colour blind, that is, one may have a much better hue perception than another. (5) Dichromic vision may be associated with defects of light perception which are also found in cases in which the vision is not dichromic. (6) Dichromics may have a perception of shade and a luminosity curve similar to the normal. (7) Many dichromics match very accurately, their colour perception being sufficient for the purpose when the colours are not too close in the spectrum. (8) The degree of colour blindness varies with the state of health. (9) Colour discrimination is diminished as a whole in dichromic vision. (10) Dichromic vision appears to be due to a defective power of colour differentiation probably

corresponding to an earlier state in evolution of the colour sense. (11) The two colours seen are red and violet."

His conclusions with regard to "anomalous trichromatic vision" are equally complicated (*Proc. Roy. Soc.*, B., Vol. LXXXVI, 1913, p. 164). "(1) Trichromic vision is not synonymous with anomalous trichromatism. (2) Many persons with otherwise normal colour perception make an anomalous equation. (3) Many colour blind persons (dichromics and trichromics) make an absolutely normal match with no greater mean deviation than the normal. (4) Colour weakness is not characteristic of anomalous trichromatism but of trichromic vision. (5) Anomalous trichromatism and colour weakness are not synonymous. (6) A large mean deviation indicates colour weakness. (7) Anomalous trichromatism appears to be due to an alteration in the normal relations of the response to the three colours (lights) used in the equation. If the eye be more or less sensitive to one of the components of the mixed colour whilst the other has its normal effect, an anomalous equation will result. An anomalous equation will also result when the yellow is more allied to green or red than is normal."

If all these statements are valid we may well despair of arriving at any concordant classification of the anomalies of colour vision; their discrimination must be purely empirical and open to innumerable fallacies. No single theory can cover so many complex variables, and investigation is reduced to the mere cataloguing of observations by empirical methods. Such collections of facts are but the raw material for the devising of theories. It is a misuse of terms to call an inchoate collection of facts a "theory," but it is only by adopting this solecism that Dr. Edridge-Green can substantiate his oft-repeated statement that he has never yet discovered a fact which is not explained by his "theory," though he has made diligent search. Professor Starling, indeed, seems to realize this aspect of Dr. Edridge-Green's work. In the 1912 edition of his "Principles of Human Physiology," he wrote: "It is very difficult, however, to harmonize the facts of colour blindness with either this (the Young-Helmholtz) or Hering's hypothesis. ***It is better to abandon hypotheses altogether and to adopt a purely empirical classification of colour vision as has been done by Edridge-Green.*** (The italics are mine).*

Fortunately, though colour vision is indeed a difficult subject,

* The section on Vision in the 3rd edition (1920) of Professor Starling's book is written by Dr. H. Hartridge, and is excellent. In his discussion of Dr. Edridge-Green's hypothesis (pp. 586-7), he gives this theory a trichromatic bias which, in my opinion, removes many of the objections that can be brought against it, but of which I find little evidence in Dr. Edridge-Green's own writings. I thoroughly agree with Dr. Hartridge's final statement—"With the evidence which has accumulated up to the present before us, there appears to be more in favour of Young's hypothesis than is to be found for its rivals. Further than that it is not at present advisable to go."

certain groups of facts are concordant in pointing to a hidden orderliness in the midst of the apparent chaos. Such, for example, are the facts of colour mixture and the facts which demonstrate some definite relationship between colour and luminosity. These, the best established correlations in the whole field of colour vision, are those the importance of which Dr. Edridge-Green minimizes or denies.

In these scattered remarks I have attempted to show that many of Dr. Edridge-Green's statements rest upon very doubtful foundations. Stress has been laid upon the evidence against them because he has tacitly ignored it. Dogmatism may win a Pyrrhic victory, but it is only by exhaustive study, for which few have the time or opportunity, or, it may be feared, the inclination, that a sound judgment of the intricacies of the subject can be arrived at. In controversies such as this, however, it is well to remember John Locke's wise words: "Truth certainly would do well enough if she were once left to shift for herself . . . But if truth makes not her way into the understanding by her own light, she will be but the weaker for any borrowed force violence can add to her."

THE THEORY OF VISION

BY

F. W. EDRIDGE-GREEN, C.B.E.

LONDON.

IN the BRITISH JOURNAL OF OPHTHALMOLOGY for July, Mr. J. H. Parsons has commenced his promised attack on my theories of vision and colour vision.

In the case of such a very difficult subject, which has been worked at by the greatest scientific men of the world, many hesitate to form an opinion because they do not know what can be said on the other side. An attack, therefore, of this kind which might otherwise prejudice those only partially acquainted with the subject, greatly strengthens my position when it can be shown that the arguments, many very old, are mostly based on misstatements.

In this article he has dealt with the retinal portion of my theory in which I assume that the rods are not perceptive elements, but are concerned with the formation and distribution of the visual purple. Mr. Parsons does not appear to have read my book very carefully, as nearly all the points he raises have been answered there. He states that some of the evidence in favour of the rods being perceptive elements is as follows: *Comparative Anatomy:* He states that there are certain animals, as, for instance, the tortoise which have only cones and others possess only rods. This I have

already answered in my book, page 54 : "It was formerly stated that a tortoise possesses only cones, but on examining the retina of a tortoise I found the rods and cones as definitely marked and distinct from each other as in man." This argument therefore is valueless.

Dr. Lindsay Johnson, whose work has not received the recognition in this country it deserves, informed me that in no animal had he failed to find a visual substance corresponding to the visual purple. I have spent many evenings with him examining his specimens. He also told me that he had discovered a large duct leading from the macular region to the lymphatic spaces of the optic nerve. Though this is strongly in support of my theory, and is probably true, I have not previously mentioned it as I was not satisfied that I saw it in the specimens shown.

The next portion of his attack has also been dealt with in my book, page 50. "Many physiologists have tried to assign different functions to the rods and cones, but all these theories have failed, because all the functions which were said to be the exclusive property of the rods have been found only gradually diminished in the fovea. For instance, von Tschermak, Hering, Hess, Garten, and I have found the Purkinje phenomenon, the variation in optical white equations by a state of light and dark adaptation, the colourless interval for spectral lights of increasing intensity, the varying phases of the after image, in the fovea, only gradually diminished. The complete absence of any qualitative change between the foveal and extra foveal regions is a very important fact in support of the hypothesis that the visual purple is the visual substance."

Mr. Parsons has such a biassed style of writing. He writes: "Although there is some difference of opinion on the point, most competent observers using the most refined methods agree that Purkinje's phenomenon is absent at the fovea." An explanation of this difference of opinion is given in my book, page 78. Results depend upon the intensity of the light employed; this demonstration was given before the Physiological Society and no one failed to see the Purkinje phenomenon with the fovea. Why is Mr. Parsons so reluctant to observe these facts for himself? During the many years that I was doing nothing but original research on vision and colour vision at the Physiological Institute, at University College, why did he not visit my laboratory, as I have often offered to show him the facts? Neither has he ever been present at any of the numerous demonstrations that I have given at the Physiological Society.

As I have pointed out, the Purkinje phenomenon is a photo-chemical phenomenon which is found with other photo-chemical substances, and therefore a duplex mechanism is not needed to

explain it. Mr. Parsons himself admits that Dr. Houstoun's work supports my view of a single mechanism. Mr. Parsons insinuates that those who have found the Purkinje phenomenon in the fovea, for instance, Hering, Hess, Garten, and others, are not competent observers, and do not use refined methods. This style of writing cannot be too severely condemned. I here challenge him to point out a defect in the method which I have devised and which is described in my book. How can Mr. Parsons possibly judge by reading a paper whether a particular method is the best. One advantage of attending International Congresses is that you learn from other men their opinions of the accuracy of different observers. I found that those of Hess were regarded with particular respect, and in this I quite agree. I have never found him wrong yet. His work on the recurrent image in the fovea is a particularly pretty piece of work. He shows that the recurrent image (stated by others to be absent in the fovea) appears bent and was therefore retarded. Both von Kries and Hess demonstrated their methods to me and it was quite obvious that retardation would explain von Kries's results.

Mr. Parsons states with regard to my discovery of channels in the retina, " The anatomical evidence in favour of such channels is of little value." Why should it be ? Anatomical points must be settled by anatomical evidence. It may be as well to relate how I discovered them. I was examining the outer side of a monkey's retina with Mr. Devereux Marshall, a pathologist of the eye of considerable experience. I pointed out the canals to him and asked him if by any chance they could be artefacts ; his reply was, " Most emphatically no." I then said we ought to be able to see them entoptically with our own eyes, and I there and then devised a method and saw in whirling currents the exact pattern of the canals of the monkey's retina. Anything more conclusive than this could hardly be imagined, and the fact of the canals explains so much of the work of Purkinje.

I would suggest to anyone who on reading my book finds that he cannot see some of the phenomena, that he asks another observer, but on no account tells him what he is supposed to see. I find that women are particularly good at seeing these phenomena if they are not biassed one way or the other.

In order to ascertain certain facts and test certain methods, I have visited Paris, Utrecht, Groningen, Amsterdam, Upsala, Berlin, Leipzig, Freiburg, Vienna, Bern, Budapest, etc., whereas Mr. Parsons would not even cross the road to see my facts, and yet tries to pose as an authority.

I must here repeat that it is absolutely necessary to see experiments before an opinion can be given upon them. In the time occupied by a research recorded in a few lines in my book, I could have written a voluminous compilation.

Some years ago one of our ablest physiologists advised me to go to Prof. Asher, of Bern, whom he considered the best qualified man in Europe to give an opinion on the theory of vision. I did so, and discussed the subject in minute detail. The following is an extract of a letter received from him March 11, 1920: "Your theory as to rods and cones, and their position in a photo-chemical theory seems to me very suggestive. I have always found a great difficulty in reconciling the facts of parallelism between the physical properties of the visual purple and the vision during dark adaptation with the physiological and psychological improbability of the Parinaud-Kries theory. Your views seem a way to avoid the difficulties."

The most conclusive fact against the duplicity theory is that when a colourless spectrum has been obtained, with further dark adaptation colour re-appears; in fact Burch stated that with complete dark adaptation there was no photo-chromatic interval. Spectral coloured light appeared as coloured immediately it was visible as light.

THREE CASES OF TOTAL REMOVAL OF IRIS

BY

D. J. WOOD,

CAPE TOWN, SOUTH AFRICA.

P. A., a fitter, came to me some years ago saying that he had been struck in the left eye by a piece of iron. The A.C. was full of blood, and there was a fine cut, not more than 1/8 inch long, just outside the cornea. The presumption was that the piece of iron was in the eye. A skiagram was negative, but I knew that the chip was small and thin from the very small hole. Accordingly I tried with the only magnet I then possessed, a small one, to get the fragment out. Using a fine terminal, introduced through the wound, I made three attempts, but failed. I decided to do no more, and expected the eye to do badly. To my surprise it remained quiet, and a week later the blood had more than half gone, but no iris was visible. It had in fact been entirely removed through the tiny wound and there was no foreign body in the eye at all. The most unexpected thing was that when I saw him some months later I found that I had escaped damaging the lens, and that even the vitreous was almost clear. Vision was 6/9. There was no trace of iris, and the whole lens and the tips of the ciliary processes could be seen all round.

A. B. M., a carpenter, came to me two years ago with the history that the night previously he had fallen against some wire netting and hurt his eye. It was very red, the A.C. was full of blood, and there was a scar above with iris pigment in it. I

cauterized the scar and sent him to hospital, giving him little hope, as the eye looked like suppurating.

The eye became quiet, however, and two weeks later I found that the iris was absent, and that there had been a laceration of the lens capsule. This was dealt with subsequently, and after removal of the lens, a convex glass gave him 6/9. In this case it was evident that the eye had been pierced by an end of wire and the iris hooked out, as in the first case, through a tiny opening.

D. L., a case of chronic iritis in a deaf and very stupid man, was in hospital at the same time as the preceding patient. There was no causal history except bad oral sepsis. There were "punctate keratitis," and iris bombé. I decided to do an iridectomy, and an anaesthetic was administered. Just as I caught the iris the patient gave a sudden cough, and I instantly let go. On introducing the forceps again I found that the iris was loose and it came out entire, without an apparent tear in it. There was hardly any bleeding and the lens was seen to be opaque. The eye quieted down quickly and there was no more inflammation. The lens has not yet been removed, as the other eye sees well. It would appear that in some cases of obstinate chronic iritis the total removal of a thickened iris is not merely easy, but is very effective as a cure.

DETACHED RETINA

BY

D. J. WOOD,

CAPE TOWN, SOUTH AFRICA.

THIS case was lost sight of and could not be published in time for the committee of the Ophthalmological Society which reported a few years ago.—Klaas Faure, a boy of twelve, was under my care in September, 1912. His left eye had been blind from detached retina for two years. He gave the history of a slight injury to the good eye five months ago and sight had got bad a month later. He could count fingers at three metres. In the upper part of the right fundus there was some scarring, and much pigmentary change, some similar change on the outer side, a chain of vitreous opacities in front, and a large grey and apparently oldish detachment in the lower part of the fundus. After a week's rest I did a scleral puncture below and removed some clear yellowish fluid. After another week I was unable to satisfy myself that any retina remained undetached except a small area nearly out of view. As I was not able to give the father a definite promise of success he took the boy out of hospital, much to my regret. It was reported to me that in 1916 the boy was able to see well, and in the same year I saw him.

The retina was in place everywhere but the lower part like the rest was covered with irregular changes, pigmentary and absorptive. There was a band of vitreous striae pointing to the old puncture but its site could not be determined. —2.25 D.cyl. vertical gave 3 letters of 6/12. I saw him again in March, 1919, and found him the same, except that he was developing striae on the back of his lens. He saw 6/18 and said that he had no trouble in seeing and could shoot partridges. His field for a small object was very irregular, but with a big object it was apparently quite full.

The occurrence of late cataract in these cases is, in my experience, common. It occurred in one of the cases reported to the committee and has also appeared in the next case, which I report as an example of a line of treatment which I described at a medical congress at Johannesburg in 1910 but which seems to have been since taken over by Birch-Hirschfeld.

Mrs. J., aged 42. Seen in December, 1913. Left eye with $\frac{-14 \text{ D. sph.}}{-1 \text{ D. cyl. hor.}}$ sees 6/9. Right eye had been failing a month, and shewed a large detachment below, not reaching the centre, with a clear vitreous. After a week's rest I punctured the sclerotic below, and passing in a fine hypodermic needle, half filled with sterile saline, I aspirated off some of the fluid, and partially withdrawing the needle reinjected it with some of the saline into the vitreous. The fluid was nearly colourless, and very little reaction occurred. This was done on December 10, and on January 7 I was able to see that the retina was all in place. She had to leave soon after this, and with —8 D. sph. got 6/60, this poor result being due apparently to central retinal damage, which I did not at first detect. I saw her again this year, and found good tension and projection, but the lens was too opaque for internal examination.

I should like to recommend this method of treatment for trial, though its application is limited. I have treated four cases up to the present, extending over a number of years. The main indication that I have gone upon has been the accessibility of the detachment. Those which do not come well forward present too many difficulties in technique to incline me to try.

The great difficulty has been that no hypodermic needle can be made to puncture the sclera without altogether too much force being used. I have had to make a puncture with a Graefe knife and then to pass in the needle. This is nearly impossible if the puncture is far back as one cannot get a clean sclerotic, and unless it is bare and free from haemorrhage, one cannot find the hole.

The use of saline is, of course, an attempt to plump up the eye more than can be done by merely shifting fluid from behind the retina into the vitreous. In none of the cases was there any bad result, but two were not helped.

In the first case—an only eye, nearly blind—I believe I merely reinjected the fluid under the retina, as the detachment was very complete. In the second case I attempted to penetrate the retina through the vitreous, and succeeded, but the fluid was very dark in colour, and much vitreous opacity resulted. The third case was apparently quite successful, as the retina seemed in place after two weeks, and the vitreous was fairly clear. In this case, as in Mrs. J's, the subretinal fluid was very pale and when mixed with the saline in the syringe was almost colourless. Unfortunately, the patient had to leave and never kept his promise to return.

As regards the operation: if the sclerotic is well exposed and bleeding has been stopped, there is not much difficulty in passing a needle through a puncture in the sclerotic. A very efficient all glass syringe is essential, and I think a little sterile vaseline on the piston is useful as preventing it from slipping back and aspirating air before it is inserted. I started with about five minims of normal saline in the syringe and injected this with what one removed from beneath the retina until there was a distinct sense of tension. I have never been able to follow the needle inside the eyeball by using a mirror, but possibly efficient help might enable one to avoid the wrinkling of the cornea which is the main difficulty. The needle can be introduced till it touches the sclera and one should then be well under the retina. One has, however, less assurance that while withdrawing it the retina does not follow, and cause one to reinject the fluid into the subretinal space again. The idea which prompted one to try the method was of course that the fluid, if clear, was an organic fluid, and would possibly set up little irritation in the vitreous, and be more permanent than saline alone. I should, however, avoid in future making use of darkly tinted fluid, and my impression is that where one meets with this that the fate of the eye is certain blindness. Two successes out of four cases, none of which was promising, seems to show that the method is likely to be useful, and if it has any merit it may be worth recording that it was not done first in Germany.

ACCOMMODATIVE FAILURE IN MALARIA AND *INFLUENZA*

BY

D. J. WOOD,

CAPE TOWN, SOUTH AFRICA.

WE are all familiar with the ocular paralysis of diphtheria, with the temporary ciliary weakness of nursing women, and with the premature presbyopia of cases of elementary toxaemia, but within

the last two years I have been brought in contact with numbers of cases of malaria from what was then German East Africa, and with a devastating epidemic of influenza, both of which have shewn me that a marked cyclo-paresis may occur in these two diseases.

In the malaria cases there were beside the complaint of headache, two ocular troubles, inability to stand sunlight and inability to read. These complaints were very constant in the cases of malaria sent back from East Africa. After a bad attack the tenderness of the eyes to light lasted, as a rule, for about a week, but the difficulty in reading persisted longer, a month or so if no second attack occurred to keep it going. Neutral tint glasses and an allowance for reading of a diopter or sometimes more were needed in a large number of cases where the men were well enough to go about freely.

In October, last year, South Africa was visited by influenza of virulent type, the death rolls in Kimberley and Cape Town being about 4,000 and 9,000 respectively.

After the disease had passed I began to get numbers of cases where patients were unable to read, though they were quite able to return to work. Some cases showed such complete cyclo-paresis that I was obliged to enquire carefully to assure myself that they were not post-diphtheritic. For example, I saw a girl of twenty seven who complained that her sight had been failing for nine days and had got so bad that on the morning of her visit she could not find her hairpins on her dressing table. She had had influenza with pneumonic complications a month before, and her accommodation was so poor that she could only read J. 10 at arm's length. She had only 0·5 of astigmatism and read 6/5. With the help of plus 2 D sph. she read well.

In the same week, the beginning of December, I had a second case nearly identical: she was a cashier and was unable to see to add up her own columns of figures in the afternoon. Thereafter I saw scores of cases mostly not so bad, but with quite unmistakable accommodative failure demanding temporary help. The difficulty continued for many weeks, and recovery occurred slowly. In many of the cases there occurred at the same time the loss of hair with which every one is now familiar.

In malaria probably and in influenza certainly the failure of the ciliary muscle seemed to be out of all proportion to the amount of general illness and persisted after the patient felt quite well again.

PARESIS OF ACCOMMODATION FOLLOWING INJURY BY LIGHTNING

BY

J. AUGUSTUS LEA,

GRAHAMSTOWN, SOUTH AFRICA.

W. L., aet. 37 years, was struck by lightning. He was unconscious for a few moments, but did not fall, and for a few minutes was unable to see.

When I saw him on the following day he was suffering from great pain in the eyes and photophobia; the conjunctiva of each eye was very red and congested. The skin of the eyelids was reddened, the eyelashes charred.

In about a week he recovered from the conjunctivitis. He returned after two weeks, complaining that he could no longer see to read properly with his glasses.

I had prescribed for him about three years before this time the following correction:

R.E. +0.75 D. Sph. ⌒ + 0.25 D. Cyl. ax. 80° d. and i.
L.E. +1.0 D. Sph. ⌒ + 0.25 D. Cyl.: ax. 80° d. and i.

With this correction he then read J.1 from 5½ inches to 20 inches with each eye singly.

When he came to see me this time he could not read No. J.1 nearer than 13 inches with the left eye, and 9 inches with the right eye, showing slight irregular paresis of his ciliary muscles. He was also exceedingly nervous. . . . All conjunctivitis had disappeared, but the eyes were still irritable, and after reading a short time the sight became dim and the eyes painful.

He improved with weak eserin drops locally, and nux vomica internally, and then, having to leave for another part of the country, I lost sight of him.

THE TREATMENT OF TRACHOMA

BY

J. AUGUSTUS LEA,

GRAHAMSTOWN, SOUTH AFRICA.

AFTER reading the excellent paper by Captains Stukey, Tomlin, and Hughes in the issue of the BRITISH JOURNAL OF OPHTHALMOLOGY, of January, 1920, I am induced to call attention to a method of treatment of chronic trachoma which I recommended in a paper read before the Fourth South African Medical Congress, and published in the *South African Medical Journal*, of August, 1898.

Trachoma was formerly very prevalent, more especially in the Karoo districts of South Africa; and I have had exceptional opportunities of seeing and treating cases during a long residence in this country. In the first place I may say that the appearances of the cases described in the paper of January, 1920, coincide with what I have observed in this country in every particular.

The method of treatment which I have used now during several years, and found very successful in chronic trachoma, is as follows.—I have an instrument made for me by Weiss, which is really a tattooing needle bent to almost a right angle at about 1.5 mm. from the point.

After everting the upper eyelid, and washing with a saturated solution of boric acid, I instil a few drops of Darier's "Coca-Renalin," I then dip the instrument in a mixture of equal parts of acid acet. fort., and distilled water, and prick each grey translucent granulation: then wash oft the excess of acid with the boric solution.

If the granulations are very numerous and small, instead of pricking each granulation I scarify the granulations with the charged needle. The groove of the needle being filled with acid, like a pen with ink, each prick carries a little of the acid into the granulation. I find that after this treatment the true granulations disappear very rapidly.

The treatment causes little or no pain if Darier's weak solution of cocaine is used; and the reaction is very slight, unlike the pain and violent reaction usually met with after "expression."

For the treatment of hypertrophied papillae I have found that if there is a marked discharge the application of a 2 per cent. solution of nitrate of silver is the best. If the discharge is scanty or none at all, I find that the application of sulphate of copper is the best treatment.

But in using either of these applications I have found it advisable to limit the application to the granulations only, avoiding healthy conjunctiva. To do this in using the solution of nitrate of silver I apply with a minute camel hair pencil, neuturalizing with solution of sodium chloride.

When I apply the sulphate of copper I use a very fine point of the crystal, and as far as possible touch the granulations and nothing else, washing off excess of sulphate.

I got very good results in the treatment of hypertrophied papillae by using a 10 per cent. solution of argentamin; but, unfortunately, since the War I have been unable to obtain a fresh supply.

THE GOUTY EYE

BY
J. AUGUSTUS LEA,
GRAHAMSTOWN, SOUTH AFRICA.

R. J., aet. 57 years. I have seen this patient on several occasions during the last ten years. On each occasion he has been suffering from conjunctivitis, principally bulbar, patchy, with, on some occasions, much sclerotic venous congestion. On each occasion it was one eye only that was affected: sometimes, right, sometimes left. He would complain that the eye felt hot, irritable, and full. He suffers occasionally from attacks of gout. He takes rather large quantities of food, especially animal food, and a moderate amount of spirits, and very little exercise.

In June, 1919, he came to see me again with an irritable right eye and well marked gout in the foot.

On examination with the unassisted eye, the same patchy injection of the bulbar conjunctiva near the limbus was found. On examination with a loupe I found that the patches were continued over the cornea by numerous closely packed minute vessels running parallel, like a minute fringe, over the cornea for about 3 or 4 mm. The patch of cornea in the neighbourhood of this fringe looked irregular, although not raised into distinct vesicles, but appeared as if there was fluid underneath the superficial layer. This did not stain with fluorescein. Five days afterwards the congestion at the limbus had subsided, but irregular grey dots could be seen where the surface of the cornea had been raised. These grey dots disappeared in a few days.

I have also notes of the case of a lady who suffers from occasional slight attacks of gout. On one occasion my notes say: Redness and pain in the left eye, patch of redness near cornea, conjunctival vessels running over edge of cornea; which at this point is swollen and appears irregular and crinkled.

Remarks.—These cases of gouty inflammation of the eye are described in text books under the names of "Episcleritis," "Hot Eye," Subconjunctivitis, and Episcleritis fugax.

I think that the term episcleritis should be reserved for the cases of inflammation which occur generally at some distance from the cornea over insertion of a muscle. These cases, in my experience, are generally connected with rheumatism.

The ordinary "Hot Eye" is, in my experience, a subconjunctivitis near the corneal margin, which occasionally becomes a superficial marginal keratitis, the conjunctival vessels passing over margin of cornea—and is usually a gouty disease—which I think is fairly well shown by the fact that many cases have well marked gout in the feet, and are at once relieved by colchicum.

ANNOTATIONS

The Standardization of the Illumination of Test-cards and Perimeters

Readers may remember that the preliminary report dealing with the standardization of test-cards and perimeters submitted by a joint committee of the American Illuminating Engineering Society and of the American Ophthalmological Society was noticed in these columns in 1917 (p. 757). A further report is now to be found in the last volume of the *Transactions* of the last-named Society. An account is given of some additional data that have been studied. ***The light should possess, as nearly as possible, the quality of daylight.*** For practical purposes this may be secured in the Corning "daylite" screen, which may be used with either electricity or gas as the illuminant. ***The intensity of the light should be such that, when installed, it represents the maximum of the range through which the illumination may vary without materially influencing the visibility of the test objects.*** With either electricity or gas this requirement may be fulfilled. ***It is desirable that the light should be readily controlled,*** which can be secured only when electricity is employed as the illuminant. ***The light should be uniform in intensity and evenly distributed over the surface of the chart.*** This may be met with electricity or gas, although not satisfactorily with either. ***The light should be so arranged that reflected light from the surface of the chart is deviated from a perpendicular to the chart.*** This desideratum must be met by variations of the lights adapted to the method of display of the test-objects. Other requirements are (A) ***the surroundings should be of subdued hue, and of an order of brightness not greater than that exhibited by a moderately light grey under illumination equal to that of the test chart;*** and (B) ***the chart and the patient are to be so arranged that no light-source, window, or other spot of light brightness shall be visible to the patient while he is engaged observing the test-chart.***

The committee considers as a suitable illumination equipment for a consulting room, and as adaptable for a centre where large numbers of men are examined, two "daylite" lamps of 72 watts, contained in a rectangular box, the back of which is concave within, and the front enclosed with a removable plate of frosted glass. The lamps are placed at the upper and lower thirds of the container. The inner concave back is bright, and acts as a reflector. The box may be attached to the wall by adjustable arms, so that the distance of the box from the card may be varied, and the angle of reflection of the light upon the surface of the chart altered.

The question of the illumination best suited for perimeter measurements will be reserved for a later report. The question of proper colours for the perimeter is correlated with that of proper illumination. But the committee, from a standpoint of light value, recommends the set submitted by Weiss & Sons, of London, decided upon some years ago at the Heidelberg Congress.

Medal for Research of the American Ophthalmological Society

Dr. Lucien Howe, of Buffalo, has given to the American Ophthalmological Society the sum of fifteen hundred dollars, the income from which is to provide a suitable gold medal for a thesis on ophthalmology, open to candidates, whether members or not of the Society in question. The competing essays may deal with (A) an original investigation of some phase of ophthalmology, or (B) the discovery of some new method of examination or treatment of the eyes. The gift is not offered free of some stipulations, of which the chief are: 1. That the Society shall be incorporated, a legal process that in the State of New York offers no particular difficulty; 2. That the principal be deposited in the Society's name with the Metropolitan Trust Company of New York, or some other trust company, with instructions to pay the interest annually to the treasurer of the Society; 3. That if no thesis presented during a given year is thought worthy to be awarded the medal, the interest for that year is to be added to the principal of the fund; 4. That as the money at the disposal of the Society will thus gradually increase, the Society will be at liberty to establish two or more such prizes; 6. That if the American Ophthalmological Society gives up or forfeits its certificate of incorporation, or ceases for more than three consecutive years to hold its annual meeting, the principal shall become the property of the Section of Ophthalmology of the American Medical Association.

Monument to the Memory of Brisseau

A movement has been started in Belgium to raise a monument to the memory of Michel Brisseau, at Tournai, where he was born in 1676, and a French committee has also been formed to invite subscriptions towards the desired object, and to help in every way the work of their Belgian colleagues. Brisseau was the first to demonstrate the seat of the opacity in cataract. As readers know, he performed depression upon the eye of a dead soldier affected during life with cataract, and then opening the eye found that the opacity he had depressed into the vitreous was in fact the crystalline lens. When he placed his observation and conclusions before the

French Academy in 1715, he was confuted by Galen's views as to the nature of cataract. The Academy refused to recognize the doctrine until three years later, when Brisseau brought forward new evidence as to the truth of his view. Brisseau thus prepared the way for Jacques Daviel (1745) to devise the operation for cataract by extraction of the crystalline lens. The question has been often raised whether Michel Brisseau was a Frenchman or a Belgian. The controversy may now be regarded as closed by a recent article (*Arch. d'Ophtal.*, July, 1920), in which Professor van Duyse shows that Jacques, the father of Michel, was born at Paris, while Michel was born at Tournai, at a time when that city belonged to France (1667 to 1709). It is of more consequence to note that the Belgian and the French Ophthalmological Societies have now united in honouring the memory of Brisseau.

OXFORD OPHTHALMOLOGICAL CONGRESS, 1920

The Eleventh Annual Meeting of the Oxford Ophthalmological Congress was held at Oxford on July 14th, 15th, and 16th last. The proceedings took place in the Department of Human Anatomy of the Museum, kindly lent for the purpose by Professor Arthur Thomson, whilst members once more enjoyed the hospitality of Keble College.

The attendance was large and included a number from overseas, prominent amongst whom were Professor Harvey Cushing (Boston), Dr. Luther C. Peter (Philadelphia), Professor van der Hoeve (Leyden), Dr. Holth (Christiania), Dr. S. Lewis Ziegler (Philadelphia), Dr. W. Hardin Sears (Huntingdon, Pa.), Dr. Bently (Seattle), Dr. Gjessing (Drammen), and others.

The chief features of the Meeting were, on the first day, a discussion on "Perimetric Methods," and on the second day the Doyne Memorial Lecture.

The former was opened by Dr. Luther C. Peter, who limited his remarks to the newer methods of perimetry and the type of cases for which they were especially adapted. After pointing out the inefficiency of the ordinary perimeter in recording changes in the central zone, the opener proceeded to discuss the various modern instruments specially adapted for this purpose. Dr. Peter then urged a change in the nomenclature from linear measure in millimetres to angle subtended in degrees or minutes, and discussed the size of the test object, especially for colour studies. The size for the central zone should subtend an angle of half a degree and that for the peripheral zone one degree. The special methods required for the study of bilateral central scotomata were (*a*) by

making use of muscle sense, and (*b*) by the speaker's method of combining perimeter and campimeter.

A good discussion followed in which the following took part: Professor van der Hoeve, Dr. A. H. H. Sinclair (Edinburgh), Dr. Marx (Leyden), Professor Harvey Cushing, Dr. S. Holth, Mr. E. H. E. Stack (Bristol), Lt.-Col. R. H. Elliot (London), Dr. A. Maitland Ramsay (Glasgow), Major A. E. J. Lister (I.M.S.), Mr. T. Harrison Butler (Leamington Spa), Mr. Burdon-Cooper (Bath), Mr. E. E. Maddox (Bournemouth), and Mr. P. G. Doyne (London). Dr. Marx showed an ingenious test object for use with his perimeter, consisting of a small steel ball moved by an electro-magnet applied to the back of the screen, whilst Dr. Holth exhibited three patterns of a chord pocket perimeter of his own design.

The Doyne Memorial Lecture was delivered by Mr. F. Richardson Cross, J.P., LL.D., F.R.C.S., of Bristol, the subject being "The Nerve Paths and Centres concerned with Sight." The lecturer, after paying a fitting tribute to the founder of the Congress, the late Robert Walter Doyne, dealt in a comprehensive manner with visual tracts and centres, comparing certain areas with the homologues in monkeys, birds, and reptiles, etc. A number of interesting slides were shown of the field of vision in quadrantic hemianopia and cases of injury of the occipital cortex, bearing out the more recent views on the relationship between the peripheral visual fields and the centres in the occipital area. At the conclusion the Doyne Memorial Medal for 1920 was presented to Mr. Cross by Mr. Sydney Stephenson, the Master of the Congress.

In addition to the above, papers were read by Mr. A. S. Percival (Newcastle-on-Tyne) on "Light Sense, with some practical deductions drawn from its consideration;" Professor van der Hoeve on "Eye Symptoms in Tuberose Sclerosis of the Brain," an extremely interesting and valuable contribution to the present knowledge of a rare disease; and by Mr. Maurice Barton, "An Examination of the Eyes of Pit Ponies, particularly with reference to Miners' Nystagmus."

Mr. A. Hugh Thompson read a paper on "Physiological and Glaucoma Cups" in conjunction with one by Mr. Rayner D. Batten entitled "Premonitory Symptoms of Glaucoma: 1, Deep Tension; 2, Excavation of the Optic Disc."

Dr. S. Lewis Ziegler's paper on "The Ocular Menace of Wood Alcohol Poisoning," attracted considerable attention, as also did a fascinating contribution by Dr. Louis Sambon on "Ancient Eye Instruments."

All the papers were discussed, though, unfortunately, owing to the length of the programme, less fully than the several subjects merited.

On the afternoon of the second day the Congress adjourned to

the Eye Hospital where short papers were read on operations, new instruments, and ophthalmic apparatus, as follows:

Lt.-Col. H. Herbert on "The Indications of Glaucoma Operations," four illustrative cases being shown.

Dr. S. Holth described a new punch-forceps for sclerectomy in glaucoma.

Mr. N. C. Ridley showed a patient whose orbit he had successfully restored by a plastic operation and who was consequently able to wear an artificial eye.

Dr. Rayner D. Batten demonstrated the Hydrophthalmoscope on a patient and indicated its value clinically.

Mr. A. S. Percival showed some simple devices for estimating the light sense.

All the contributions were freely discussed.

In the Scientific Museum, which was open during the Congress, Mr. E. H. E. Stack (Bristol) showed a simple and ingenious motor trephine which should prove of considerable aid in the operation of trephining. The author is to be congratulated on the device.

Mr. R. T. E. Hanson, O.B.E. (London), showed a well-designed ophthalmic bureau and equipment for R.N. oculists serving at either a Royal Naval Hospital or a Dépôt, a Naval Base, or a Fleet at sea, and also drawings and models of improvements in gun-sighting telescopes, theodolites, and microscopes.

Mr. E. E. Maddox (Bournemouth) showed (1) an artificial epistaxis knife, (2) illuminated forceps, and (3) the "V" test for astigmatism.

Mr. N. C. Ridley showed a new cataract knife.

In a separate room Professor Barr's optophone, which has recently attracted so much attention, was demonstrated to members.

In the Commercial Museum instruments, apparatus, lenses, and books were on view during the meeting.

At the close of the proceedings on the first day a General Meeting was held, after which members with their friends met at Queen's College for tea, the Rev. S. B. Cronshaw acting as host.

The Official Dinner of the Congress took place on Thursday evening, the 15th, in the Hall of Keble College.

ABSTRACTS

I.—REMEDIES

(1) **Abadie, Charles (Paris).—A new method of treatment of severe and inveterate forms of granular conjunctivitis. (Nouvelle méthode de traitement des formes graves et invétérées de la conjonctivite granuleuse.)** *La Clin. Ophtal.*, November, 1919.

(1) It is astonishing with what enthusiasm new methods of treatment of intractable diseases are advocated by their authors. How seldom are the hopes held out of an absolute method of cure justified by after-results. What will be the verdict of others upon **Abadie's** new method of treating the more severe forms of trachoma?

Premising that the disease is microbic and that the intractable cases are due to the invasion of deeply-seated submucous tissue extending back towards the orbital tissue, Abadie has devised and used on a large scale in France, Spain, and Morocco, a method of cauterization of the deep cellular tissue extending from the superior cul-de-sac towards the orbit. A strong galvanocautery with a sharp and rigid point is employed. The upper lid and cul-de-sac is completely everted by means of a special forceps, the globe is protected with a thin metal plate and then, at about three millimetres from the external commissure, the cautery at a "red-white" heat is plunged into the conjunctival mucosa and the subjacent tissue, keeping as close to the globe as possible. At about 3 to 4 millimetres of depth the cautery becomes cooled and is withdrawn. It is heated up again and once more plunged into the same spot. This is done a third time and the pit which has been formed is then about a centimetre deep. Three or four millimetres from the first pit a second is made in the same way, and so on till there are four or five pits of about a centimetre in depth extending between the two ends of the everted lid. The surface is then simply bathed with a solution of perchloride of mercury, 1 to 500. The author states that there is no reaction and that no dressing is necessary. Bathing with boiled water is all that is required. The patient often obtains immediate relief of symptoms. As the reviewer understands the author, the upper lid is forcibly everted every two days, and the region of the perforations bathed with sublimate, 1 to 500 as above. At the expiry of two or three weeks the conjunctival surface has become normal, and pains, pannus, corneal ulcerations, etc., have disappeared as if by magic, and the disease is rapidly becoming cured. "I have operated," says Abadie, "by this new method on numerous patients who have dragged out a weary existence from hospital to hospital for years past, and have undergone the most varied forms of treatment without any definite success; and they have been completely cured."

ERNEST THOMSON.

(2) **Key, Ben Witt (New York).—Antidiphtheritic serum in severe ocular infections, with special reference to hypopyon keratitis.** *Trans. Amer. Ophthal. Soc.*, Vol. XVII (1919), p. 470.

(2) This long paper (55 pages) by **Key**, accepted as a candidate's thesis for membership of the American Ophthalmological Society,

deals with paraspecific therapy. Both sides of this vexed question are set forth with impartiality, an account of thirty cases (of which twenty-three were instances of hypopyon keratitis) treated by anti-diphtheritic serum is given, and the author presents the following conclusions:

"1. Literature on paraspecific therapy is an interesting study and research, and clinical reports of wide experience point towards a field worthy of being explored.

2. Favourable results with paraspecific therapy are by far in the majority over unfavourable reports.

3. Antidiptheritic serum seems to have an advantage over the other paraspecific agents, because it is readily obtained, its dosage is more certain, and the preparation more dependable.

4. It has the advantage over specific serums and vaccines in that it can be utilized during the time when a bacterial diagnosis is being made and the special vaccine or serum is being prepared.

5. Indications are: in all severe infections due to pneumococcus and staphylococcus, especially hypopyon keratitis, phlyctenular pustule, penetrating wound with infection, critical ulcer of cornea, post-operative infection, and panophthalmitis.

6. It should be administered early in the course of the infection, though its effect upon the late stages is even more striking than in earlier involvement.

7. Administration by hypodermic seems theoretically more practical, though its use by mouth has been strongly advocated with results equally as good.

8. Dosage is 5,000 to 2,000 units, the former infrequently repeated, the latter every day for three or four doses.

9. Adverse effects seem to have been of no importance.

10. Its principal effect is probably systemic, and to some extent reduces the virulence of the invading organism, through the production of antibodies.

11. Its curative properties are demonstrated by prompt effect upon hypopyon, reduction in conjunctival and iritic reaction, clearing away of ulcer débris, and relief of pain.

12. Its effect is limited by the presence of syphilis, untreated, and probably also by other systemic conditions.

13. It cannot be depended upon solely to combat an infection, though it may be the principal means of effecting a cure. Local measures, as indicated, are essential.

14. The time of injection in relation to the stage of involvement and the time of employment of other treatment may become a matter of special consideration and value when more is learned about paraspecific therapy.

15. The prophylactic use of antidiphtheritic serum at this time is only problematic." S. S.

(3) **Luedde, W. H. (St. Louis, Mo.)—Notes on vernal conjunctivitis.** *Trans. Amer. Ophthal. Soc.*, Vol. XVII (1919), p. 717.

(3) **Luedde** presents a kind of general review of that curious affection vernal conjunctivitis, in the course of which he brings out nothing very new. As regards treatment, Luedde admits he has not personally tried radium or X rays, but his experience with a simpler remedy, fibrolysin and weak solutions of silver or zinc, has yielded results of a satisfactory nature. Before the fibrolysin is applied, a 1 per cent. solution of holocain is put into the eye. Two or three applications a week of fibrolysin are sufficient to produce a freedom from discomfort even in severe cases and during the hottest weather. In some cases it seems that treatment at intervals of one month throughout the year can prevent relapses and bring about a cure. It is unfortunate that throughout Luedde's communication no clear statement can be found as to the exact strength of the fibrolysin employed or the way the application is made to the eyes.

S. S.

(4) **Weekers, L. (Liège).—The internal treatment of glaucomatous hypertension.** (**Le traitement interne de l'hypertension glaucomateuse.**) *Bull. de la Société belge d'Ophtalmologie* (1920), p. 39, and *Arch. d'Ophtal.*, July, 1920.

(4) **Weekers** sets himself to answer the question, is it possible to influence glaucomatous hypertension by means of medicaments given internally or injected under the skin or into the veins? In 1912 he published an account of the favourable results obtained by means of calcium chloride, results confirmed by Gowland (1917) and by Alt (1919). Weekers has tried the method on 21 cases of glaucoma, and checked the results by tonometric observations recorded at regular intervals. Among most of the patients the treatment failed to influence the intra-ocular pressure, although in a few it had a favourable influence in that direction. He administered three grammes of calcium chloride a day, and continued the treatment for twenty or thirty days. He now details five successful cases, all of them instances of chronic glaucoma, but we note that in two of them sclerectomy was performed eventually. In three of the cases pilocarpin was used at the same time that calcium was being given. The calcium chloride acts especially in cases of chronic glaucoma. In acute glaucoma, on the other hand, the digestive tract bears the calcium salt badly or not at all, so that one is driven to give it by intramuscular injection and consequently in inadequate doses.

S. S.

II.—ORBITAL INFLAMMATIONS

Wissmann, R. (Strassburg).—A contribution to the study of the symptomatology and treatment of orbital inflammations. (Beiträge zur Klinik und Therapie orbitaler Entzündungen.) *Arch. f. Ophthal.*, Vol. XCVII, 1918.

Wissmann gives a description and analysis of a series of cases of inflammation of the orbit, classifying them according to two main types, (*a*) those connected with processes arising in the walls of the cavity (subperiosteal, periosteal and osteal inflammation and suppuration), and (*b*) those arising in the retrobulbar tissue itself (diffuse infiltration, retrobulbar abscess, phlegmon, thrombophlebitis) which differ from the point of view of diagnosis, prognosis, and treatment; it may not always be possible to draw a sharp line between the two and one type may pass into the other, but there is frequently sufficient evidence to guide one to a correct diagnosis in any particular case.

Of the first group the author gives details of eight cases, the majority of which arose from suppuration of one or more of the nasal accessory sinuses: one case was due to erysipelas, and another to bacterial infection of the bone leading to subdural abscess.

The clinical symptoms—exophthalmos, displacement of the globe with limitation of movement, localized tenderness of the orbital margin and the position of the swelling in the lids—are sometimes so characteristic as to indicate the site of the abscess and the sinus affected, and the appropriate treatment of the case. It is, however, not always easy to determine the sinus that is affected: the course of two of the author's cases convinced him of the advisability of opening up the posterior ethmoidal cells where they are suspected, in cases in which the symptoms are very urgent, even when the rhinological examination is negative. Treatment was successful in all but one case.

The second type is illustrated by seven cases. In these the clinical signs are intense doughy swelling and infiltration of the lids extending over the cheek (resembling erysipelas), great chemosis of the conjunctiva, limitation of movement of the globe (not always very marked) and, more especially, grave general disturbance with high temperature, the rapid development of the disease and marked protrusion of the eye in the line of the orbital axis.

The majority of his cases were due to an infection of the skin (furuncle of the face or trauma) setting up phlebitis and thrombosis of the orbital veins. The accessory sinuses were examined in every case, but in all but one were found to be free from disease.

The absence of any definite localizing sign in the orbit makes the treatment of these cases more difficult. Conservative measures or

merely superficial incisions are futile. In his cases deep incisions were made along the orbital margin so as to allow of a search for subperiosteal abscess, but while an abscess was never found, small foci of pus, both in the lids and orbital tissues, were encountered so regularly as to become a sign of some importance for differential diagnosis. Exenteration of the orbit, as suggested by Hertel, was also done in some of the cases, and, where it was carried out sufficiently early, gave successful results—in two cases. The circumstances of each individual case must decide the time for adopting this method of treatment.

THOS. SNOWBALL.

III.—THE ABILITY TO READ WITHOUT ACCOMMODATION IN THE APHAKIC EYE

Davis, A. Edward (New York). — Accommodation in the lensless eye occurring in a woman aged seventy-three years, following extraction of cataract. *Trans. Amer. Ophthal. Society*, 1919, p. 444.

Förster, Woinow, von Graefe, Silex, Jaeger, Living, Davis, Collins, and others maintained that accommodation power, in exceptional cases, was present in the lensless eye, whereas Donders, Helmholtz, and Manhardt opposed this contention. Davis now reports the case of a woman, aged 73 years, who had a cataract removed from one eye by the combined extraction. Six weeks after operation L.V. with +16.00 D. sph. and +1.5 D. cyl. axis 150° = 15/30. The patient read J1 at from 7 to 13 inches with the same correction without tilting or changing the position of the glass. The pupil was closed by a membrane, except for a clear space at the centre, vertically oval in shape, 2.5 mm. by 3.5 mm. in diameter. Many tests, both of a subjective and an objective character, were made whereby the possible influence of lid pressure and other fallacies appear to have been eliminated. After atropin had been used for three days it was found that the near point had receded one inch from the eye, which would indicate that in Davis's patient the ciliary muscle had some influence (0.71 D.) on the accommodation even after the lens was removed. Davis concludes that the accommodation apparently present in the lensless eye is due chiefly to the ability of the patient to interpret dispersion circles, and this he traces to the following causes (*a*) to the great increase in size of the retinal images by removal of the crystalline lens and replacing it by a lens in front of the eye, (*b*) to the narrowing of the pupil, (*c*) to the patient not looking directly through the centre of the glass, but slightly outside the centre, as by tilting the glass, tilting the head,

or not holding the reading matter directly in front of him, (*d*) to the extraordinary acuteness of vision sometimes present after cataract extractions. In Davis's opinion, the last factor but one of those named (*c*) assists the patient in interpreting dispersion circles chiefly by neutralizing the monochromatic aberration present in such cases.

The discussion that followed the reading of Davis's paper brought out the fact that these cases are not so uncommon as might be thought. Several speakers laid stress upon the influence of a pinhole pupil in producing a condition such as that described. **Lambert** (New York) thought that the explanation lay in the action of the extrinsic ocular muscles in producing a change in the antero-posterior axis of the globe and possibly a change in the curvature of the cornea. The theory of a stenopaic opening did not explain any of the numerous cases he had seen in young subjects. **Zentmayer** (Philadelphia) defended Donders's contention, that loss of the lens implied loss of accommodation. **Jackson** (Denver) made the point that accommodation had acquired a definite meaning, and that the cases discussed were not of vision with accommodation in the ordinary sense and acceptation of the word, but without accommodation by means of compensatory devices and tricks.

S. S.

IV.—MISCELLANEOUS

(1) **Clapp, C. A. (Baltimore).—A study as to weight and percentage of solids of cataractous lenses.** *Arch. of Ophthal.*, Vol. XLV, No. iv, November, 1916.

(1) **Clapp** has weighed a number of cataractous lenses after extraction, and estimated the amount of solids each contained. The lenses showed all manner of variations met with in ordinary practice. Unfortunately, so far as the purpose of the investigation was concerned, the lenses were not in all cases extracted in capsule. The heaviest lens weighed 0·2418 grm. and the lightest 0·0988 grm. In the latter case the nucleus only was obtained and the cortical substance was lost. The average weight was 0·1640 grm. The lenses of persons over 80 years of age showed an average weight of 0·1968 grm. and also a slight increase in percentage of solid, 29·31 per cent. Those of patients under 60 years of age showed an average weight of 0·1335 grm. Their average percentage of solids (one exceptional case being left out) was apparently 27 per cent. or rather less: the exact figure is not quite clear. The percentage of solids in infant lenses was 27·57 as compared with 27·02 for all senile cataracts. The findings of previous workers are alluded to.

R. H. ELLIOT.

(2) **Wang, Chung Yik (Edinburgh).—The incidence of bovine infection of tuberculosis in children.** *Edinburgh Medical Journal*, March, 1917.

(2) In view of the probable importance of infection by the bacilli of bovine tuberculosis in such affections of the eye as Parinaud's conjunctivitis and phlyctenular disease, we may profitably glance at this communication by **Wang**, which deals with the incidence of bovine infection in children, especially in Edinburgh.

In Edinburgh it has been shown by several workers that bovine infection accounts for an unusually high percentage of the cases of tuberculosis of the bones, joints, and cervical glands in children of the hospital class. Wang examined the bodies of twenty children dying under 16 years of age at the Hospital for Sick Children, and found that eleven (or 55 per cent.) were infected with the bacilli of bovine tuberculosis. Taking all the Edinburgh cases (281) so far reported by different writers, bovine infections at different ages come out as follows:

Under 5 years	78·4	per cent.
Between 5 and 16 years	70·3	„
16 years and over	7·8	„

By means of tables, Wang contrasts the incidence of bovine infection in children under 5 years of age in Edinburgh and in England and other countries. The figures are as follows: in Edinburgh, 78·4 per cent.; in England, 31 per cent.; and in other countries, 18·1 per cent. The corresponding figures for children from 5 to 16 years of age are:—70·3 per cent.; 21·3 per cent.; and 24·5 per cent. For adults 16 years of age and over, the figures are 7·8 per cent.; 7·4 per cent.; and 0·7 per cent. Wang makes the generalization that human tubercle bacilli enter the body by the respiratory tract, while bovine bacilli are introduced through the alimentary canal. He traces the somewhat remarkable frequency of the infection in Edinburgh, first, to the cow's milk having been infected with living tubercle bacilli; and, secondly, to the fact that the feeding of the child must have consisted of, or have been supplemented by, tuberculous milk consumed raw.

Wang concludes.—(1) That a bacteriological investigation into 281 cases of various clinical forms of tuberculosis in Edinburgh has resulted in the isolation of bovine tubercle bacilli in 80 out of 102 cases (78·4 per cent.) under the age of 5 years. (2) That the most common tuberculous affections are abdominal tuberculosis and tuberculous meningitis, being together responsible for about 90 per cent. of the summed mortality from tuberculosis in children under 1 year, and for about 75 per cent. in children between 1 and 5 years. The material isolated from nine children dead from those two diseases was examined, and from six the bovine type of tubercle bacillus was isolated. (3) That a large percentage of children in

Edinburgh is fed wholly or partly on cow's milk, and that in a great number of instances the milk is consumed raw. These two facts, together with the high percentage of tuberculous cow's milk, give an explanation of the prevalence of abdominal tuberculosis in Edinburgh. (4) Children fed on raw cow's milk and those fed on boiled cow's milk, were tested with tuberculin, and it was found that 37·5 per cent. of the former reacted, as against 15·4 per cent. of the latter. (5) The remedy is to be found in an adequate supervision of the milk supply, or, failing that, in sterilization of all cow's milk.

S. S.

(3) **Molinie.—Ocular reflexes of auditory origin.** *Revue de Laryngologie, d'Otologie et de Rhinologie*, September 15, 1916.

(3) **Molinie** draws attention to the following ocular reflexes of auditory origin:

1. In a subject whose hearing is normal, a short and unexpected sound, even of feeble intensity, near the ear produces slight or complete and sudden contraction of the orbicularis palpebrarum (blinking).

2. This phenomenon is sometimes associated with a slight contraction of the pupil, followed by dilatation. The constancy of this reflex is difficult to estimate owing to its short duration and feeble amplitude and the concomitant contraction of the eyelids.

3. If instead of a short sound a continuous and increasing sound is employed, repeated alternations of miosis and mydriasis may be observed.

4. If the sound is short and very loud, the blinking is more energetic and is accompanied by contractions of the frontalis and the whole of the facial muscles.

5. If the noise is very violent (e.g., gunfire, blast of a trumpet) there are added to the above phenomena, movements of the eyeballs, of the head and neck, and sometimes a turning of the whole body towards the source of the sound.

6. In deaf subjects these phenomena are enfeebled or even entirely absent.

These reflexes are set up by a disturbance of the accommodative function of the ear—the muscles concerned being the tensor tympani and the stapedius. The stapedius is the muscle which restricts the effect of loud sounds, and it is this muscle, supplied by the seventh nerve, which is concerned in these ocular reflexes. The centres for auditory accommodation as well as those for ocular accommodation are situated in the anterior corpora quadrigemina to which the cochlear stimulus is conveyed. From this centre the stimulus passes along the posterior longitudinal bundles to the nucleus of the facial, which transmits the impulse to the stapedius. If this motor

impulse is ever so little intense, it will radiate to the other branches of the facial and give rise to the palpebral reflex or contraction of other facial muscles. When the noise is very loud, the stimulus spreads from the seventh to neighbouring nuclei, and through association fibres even further afield. The proximity of auditory and ocular centres of accommodation in the corpora quadrigemina explains the production of the pupillary reflex. The reflex movements are calculated to ensure the protection of the organ and the individual, but they also indicate the anatomical and functional integrity of the labyrinth and the cochlear nerve, and as such are a valuable means of testing the auditory nerve. For precision it is necessary to take the reflex due to a minimal stimulation. This would naturally be the contraction of the stapedius, but as this is impossible to observe, we are compelled to observe the associated reflex. Possibly the pupillary reflex is the next most sensitive, but this again is difficult to observe, and so for practical purposes the palpebral reflex is found most convenient, and is also reliable and sensitive. This reflex the author has named the "auditory-palpebral reflex."

As an excitant the author at the present time uses his *acoumètre*, which is a tuning fork, which can be made to produce a short note whose intensity can be varied at will.

The practical applications of the test are numerous, but the author confines himself to its use in military cases, and especially in labryrinthine disturbances, in which it has been assumed that there has been a haemorrhage into the labyrinth or laceration of the fibres of the auditory nerve. These lesions have not been demonstrated anatomically, and the author is inclined to believe they are of extreme rarity. The diagnosis of labyrinthine disturbance is almost exclusively made in cases of intense deafness in subjects who have received little or no direct injury, but have been near violent explosions or have witnessed tragic or distressing sights. On the contrary, this form of deafness does not occur in those who have been severely wounded and exposed to every form of disturbing effects from projectiles—auditory concussion, displacement of air, blowing up, burial, multiple wounds, etc. A selective eflect on the labyrinth of the uninjured, and immunity in the ease of the severely wounded, is in itself a suspicious anomaly, but such an hypothesis is definitely disproved by the application of auditory-palpebral reflex test. In all these cases of labyrinthine disturbance, the palpebral reflex is positive even with stimuli of feeble intensity, which shows that the auditory apparatus and reflex centres are intact. The lesion, therefore, must be one of the cortical auditory centre, or when an organic lesion of that centre can be excluded, it must be regarded as a failure of the conscious representation of auditory impressions—a suppression in the higher psychic centre. This

differential diagnosis is important, since an organic lesion of the labyrinth indicates more or less permanent deafness, whereas psychic deafness is harmless and curable by encouragement, suggestion, and such means.

The auditory palpebral reflex furnishes a valuable means for the diagnosis of simulated deafness when psychic deafness can be eliminated. Its absence will at once indicate an organic lesion; its presence, especially with a feeble stimulus, indicates a hearing capacity which it will be useless for the patient to deny. This test also supplies us with an objective sign of the hearing capacity of the patient at varying distances.

J. JAMESON EVANS.

(4) **Landolt, E. (Paris).—A simple explanation of skiascopy. (Une explication simple de la skiascopie.)** *Arch. d'Ophtal.* Jan.-Feb., 1916.

(4) **Landolt** gives in this paper an account of the principles underlying the use of skiascopy which should be intelligible to those whose knowledge of mathematics is neither extensive nor peculiar.

As he points out, it is not the shadow that we really observe, but the light in the pupil, and suggests that the correct name should be not "skiascopy" but "korelampsiscopy," or, if that word is too long, "korescopy." The usual term retinoscopy has nothing to recommend it, since whatever we are observing, it is certainly not the retina.

The explanation is made quite clear by means of coloured diagrams, and should be of use to those who have to instruct students in refraction work.

E. E. H.

(5) **Harwood, T. E.—Ocular vertigo.** *Lancet*, December 16, 1916.

(6) **Ferree, C. E. and Rand, G.—Miscellaneous experiments on the efficiency of the eye under different conditions of lighting.** *Ophthalmology*, July, 1916.

(6) In this paper **Ferree** and **Rand** continue the record of their researches described in a previous paper. The experiments dealt with the comparative results of the three types of illumination: direct, semi-indirect, and indirect, and the conclusions are represented by charts. The paper does not lend itself to abstract, but two points may be referred to. Discussing the relative value of eyeshades and lampshades in preventing loss of efficiency of the eyes in prolonged work, the authors come to the conclusion that eyeshades have no value when indirect lighting is employed, but in the case of both direct and semi-indirect lighting the use of an eyeshade is advantageous. While, therefore, improvement in the

form of the illumination is the better procedure, there are difficulties in the way; for instance, in the case of employées who have no control over the lighting arrangements, and the use of an eyeshade may be useful as a provisional and immediate aid in solving the problem of bad lighting. The experiments showed that an opaque eyeshade with a white lining was better than an opaque shade with dark lining or a semi-transparent shade.

Some experiments dealt with the effects of moving picture shows. So far as they went, they tended to show that after two hours of the pictures, in a first-class house, there was no greater loss of visual efficiency than would have resulted from two hours' reading under the conditions provided by any of the lighting systems in general use.

A. J. BALLANTYNE.

(7) **Rönne.—On retino-choroiditis (Edmund Jensen). [Ueber die Retino-Chorioiditis (Edmund Jensen)].** *Klin. Monatsbl. f. Augenheilk.*, April-May, 1915, p. 455.

(7) **Rönne** gives details of two cases of this disease, which he considers so common as to be present in about 1 per cent. of all eye-patients. He agrees with van der Hoeve, that the condition may be localized in the macular area; that the nerve-fibre defects may be only relative; that the affection may be easily mistaken for optic neuritis; and that, although the choroid is the primary seat of the disease, choroidal atrophy need not result. At the same time, he cannot support van der Hoeve's view that the upper temporal quadrant of the field always escapes. This form of choroiditis is common in its late stages, and no great rarity in the acute stage. Rönne believes that all cases of acute choroiditis belong to this group. The chief symptom which differentiates this condition from disseminated choroiditis is the nerve-fibre bundle defect in the visual field. If this were more frequently looked for, the frequency of the disease would be recognized, and the diagnosis of tubercle would be made less often. He believes that in this connection tuberculin therapy has obtained an easily acquired success, since spontaneous healing is a constant feature, and is personally convinced that this affection is certainly not tuberculous.

In conclusion, Rönne urges the collection of a large number of clinical records, with a view to the study of this form of choroiditis, and the differential diagnosis between it and optic neuritis, choroidal tuberculosis, and disseminated choroiditis.

H. M. TRAQUAIR.

(8) **Maxey, Edward E. (Boise).—Primary syphilis of palpebral conjunctiva.** *Amer. Jl. of Ophthal.*, January, 1918.

(8) **Maxey's** patient, a dentist of 23 years, gave an anaesthetic for a medical associate for a tonsillectomy in a young woman of

easy virtue. About three weeks later, one eye became inflamed. On examination, a shallow ulcer, 4 mm. by 8 mm., was found to occupy the outer part of the conjunctiva of the lower lid. It was covered with a dark greyish membrane. Fully one-half of the cornea was hidden by oedematous conjunctiva. The lids were swollen. The corresponding preauricular and submaxillary and subauricular glands were swollen and tender. Spirochaetes were not specially searched for at this time, and later, when the ulcer had been cauterized, the laboratory report in that respect was negative. The Wassermann reaction was also negative when estimated thirty-seven days after exposure, "doubtful" after forty-four days, and strongly positive after fifty-eight days. The conjunctival and other lesions soon cleared up after the patient was treated by salvarsan, mercurial inunctions, and other anti-luetic measures.

Maxey tabulates 82 cases of chancre of the lids and conjunctiva reported since January, 1900. Of this number, 49 may be classed as conjunctival chancres.

S. S.

(9) **Edridge-Green, F. W. (London).—The cause and prevention of myopia.** *Lancet*, January 26, 1918.

(9) **Edridge-Green** holds that the near work theory is not proved, which may be freely admitted. He proceeds to develop a theory of myopia causation based upon obstruction to the outflow of fluid from the eye through the posterior lymph channels. Increased intra-ocular pressure distends the sclerotic and causes myopia by elongation of the eyeball. The posterior portion of the sclerotic is a weak spot just like the inguinal ring which will give way on undue pressure. This is particularly likely to occur whilst the eye is soft and growing, but under great stress appears to occur at almost any age. Edridge-Green reproduces the conventional diagram to show the rise of pressure in a series of vertical tubes fed from a cistern, when the outflow is obstructed. The theory is interesting. But the reviewer believes that there are many flaws in the argument. It is stated that "in convergence excess, as in convergent strabismus or internal squint, myopia is *not* produced and convergent strabismus is almost exclusively found in hypermetropic eyes." Surely, this statement is much too sweeping. Again, we have this remark, "All are educated alike." Surely not, for apart from differences of education in various countries there are wide differences in schools as regards lighting, and individual differences of method. "It is very doubtful whether the use of the eyes for near work has anything to do with the production of myopia, and certainly nothing of the sort has been proved." It is, of course, easy to find faults with the usually current theory, but what has the author to say in support of his own? Essentially, that he has come across a great many

cases in which myopia appears to have come on as the result of physical strain in cycling, rowing, digging, boxing, wrestling, and so on, and a number of cases in which, he says, the ordinarily given causes of myopia cannot apply. But is there a single one of these cases, a number of which are detailed, in which the author is relying on anything but the evidence of a statement of the patient or of those who know him? In any case, supposing all these to be instances of myopia really caused by severe strain and nothing else, are they sufficient to prove a direct negative such as Edridge-Green suggests? It seems much more probable that both factors are at work. Heredity, perhaps the most certain factor of all, is only very casually mentioned by Edridge-Green. The author deserves thanks for his contribution towards the solution of a perennial problem, even though he has by no means disproved the theory which upholds the influence of near work.

ERNEST THOMSON.

BOOK NOTICES

Ophthalmic Literature (succeeding the Ophthalmic Year Book). Vol. XVI. Part 2. June 1920. Published quarterly by the Ophthalmic Publishing Company, 7, West Madison Street, Chicago, Illinois. Annual subscription, ten dollars in advance.

In the last number of this journal a notice was published concerning the *Obhthalmic Year Book*, of Denver, Colorado. The retention of this title for what had become a three-monthly publication was noted, and now, in order to meet the suggestion of the Post Office Department that Year Book is not a proper title for a quarterly publication, the name of *Year Book* has been changed to *Ophthalmic Literature*. It will contain the same class of matter as before, similarly arranged. The present number deals with the anterior chamber and pupil, the uveal tract, sympathetic disease, glaucoma, crystalline lens, vitreous humour, and retina.

S. S.

Local Anaesthesia in Ophthalmology. (L'Anesthésie locale en Ophtalmologie.) By Dr. C. DUVERGER. Paris: Masson et Cie., 120, Boulevard Saint-Germain. Price 6fr. 20 net. 1920.

Duverger, professor of clinical ophthalmology in the University of Strasbourg, has written a most useful work dealing with anaesthesia (and its history) in all branches of eye work. We

do not recall any book that covers precisely the same ground. According to the author, there are three decisive dates in the history of local anaesthesia in ophthalmology: 1. 1884, when the use of cocain was introduced by Koller; 2. 1891, when infiltration anaesthesia was advocated by Schleich; and 3. 1905, when novocain appeared.

In order to verify the data given by writers for reaching the nerves at their exit from the skull, Duverger has made certain measurements and researches on skulls and the dead subject, employing needles of 2·5 cm., 3·5 cm., and 4·5 cm. for the purpose of injecting fluids. He concludes that the 4.5 cm. needle should be reserved for total anaesthesia of the superior maxilla or for cases where oedema is present. A needle of 3·5 cm. to 4 cm. at the maximum suffices for everything else, and with such a needle it is impossible to wound the optic nerve, which can be readily done with the longer needle. Even the ophthalmic ganglion, which is bathed in a very loose cellulo-fatty atmosphere, as it were, can be anaesthetized by the 3·5 cm. to 4 cm. needle. A shorter needle should be preferred for superficial anaesthesia. Luer's glass syringe of 2 cm. capacity responds to almost every requirement. As to instillations, Duverger uses 5 per cent. and 10 per cent. solutions of cocain hydrochloride. One or two applications of the strengths named are inoffensive, and that is why the author refuses the feebler 1 per cent. or 2 per cent. solutions used by many. Cocain for injection should not be stronger than 0·5 per cent. or 1 per cent., and of such 20 cg. has been used without danger (Reclus), but Duverger prefers not to exceed half that quantity. The author describes in detail anaesthesia of particular parts of the eye and of some diseases—eyeball, cornea, conjunctiva, anterior chamber, cataract, eyelids, autoplastic operations, lacrymal sac, lacrymal gland, strabismus, frontal and ethmoidal sinuses, and orbit. Duverger recognizes two general contra-indications to local anaesthesia, namely, young children below seven or eight years, and grave infections, as erysipelas, diffuse abscess of the lids, acute peri-cystitis, orbital abscess, and thrombophlebitis. The little book concludes with the words: "we now possess adequate means for banishing chloroform from practical ophthalmology; it is a duty to have recourse to them."

S. S.

Thérapeutique Oculaire (Ocular Therapeutics). By Dr. GEORGES ROBERT. Paris: Masson et Cie, 120, Boulevard Saint-Germain. 1920. Price 6 fr. net.

This small book, which opens with a brief preface by Dr. Kalt, of the Paris Quinze-Vingts, is likely to be of service to practitioners in rendering first aid and often enough in treating affections of the

eye without the help of a specialist. If its statements break no fresh ground they are at least trustworthy. With regard to operations, the author merely considers those precautions necessary to secure a good result, besides devoting attention to the indications for operation. The book is made up of fourteen chapters dealing with conjunctivitis, keratitis, opacities of the cornea, scleritis, blepharitis, diseases of the lacrymal passages, iritis, cataract, glaucoma, wounds of the eye, intra-ocular lesions, detachment of the retina, nervous lesions affecting the eye, and the method of anaesthesia. It includes numerous prescriptions, and a good deal of attention is paid by the author to the newer remedies.

S. S.

Bulletins et Mémoires de la Société française d'Ophtalmologie. 1919. Paris: Masson et Cie.

The Transactions of the Société française d'Ophtalmologie for 1919 have just been issued. They constitute the thirty-second number; no meetings of the Society have taken place during the five years of war. Before the Annual Congress opened it was unanimously resolved that Germans, Austro-Hungarians, Bulgarians, and Turks should be excluded from membership. A report was received from MM. Galezowski, Morax, and Rochon-Duvigneaud urging the creation of an Institute of Ocular Physiology. About half the volume is devoted to the report by M. Terrien on the employment of Radium and of X rays in ophthalmology, and to the discussion which followed. The remaining portion consists of papers by various authors, from the most interesting of which abstracts will be found in this Journal from time to time.

CHARLES KILLICK.

NOTES

Death

WE regret to announce that Albert Antonelli died at Paris on June 23rd last, at the age of 55, from post-influenzal pneumonia. He was the son of Giovanni Antonelli, professor of normal anatomy in the University of Naples, the city where he was born. He established himself in Paris in 1897, acting as assistant to Dr. Landolt. He founded a large clinique at 37, Boulevard de la Chapelle, which was the centre of many activities and much work of a benevolent character. Antonelli was a Chevalier of the Légion d'Honneur and

Commander of the Crown of Italy. Perhaps the best known of his writings dealt with the rudimentary ophthalmoscopic stigmata of hereditary syphilis, the connection between syphilis and myopia, and clinical ophthalmometry. He was a member of many learned societies. In this country he had many friends and admirers, and was a member of the Oxford Ophthalmological Congress. Dr. Offret, of the Quinze-Vingts, has taken over Antonelli's clinique. We deeply sympathise with Mme Antonelli and her four children on their loss.

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Appointments

DR. A. J. BALLANTYNE has been appointed Lecturer in Ophthalmology to the University of Glasgow in succession to Dr. Maitland Ramsay, retired.

Dr. F. W. Edridge-Green has been appointed a special examiner for the sight tests by the Board of Trade. The same gentleman has been appointed Arris and Gale Lecturer by the Royal College of Surgeons of England. The subject of his lecture will be the cause and prevention of myopia.

Lt.-Col. F. P. Maynard and Dr. Aubrey Jelly have been appointed School Oculists by the Cheshire County Council.

Dr. A. E. Burroughs has been appointed specialist medical referee for ophthalmic cases under the Workmen's Compensation Act, 1906, for County Court Circuit No. 6.

Dr. H. Wright Thomson has been appointed a surgeon to the Ophthalmic Institution, Glasgow Royal Infirmary.

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Royal Society of Arts

THE Albert Medal of the Royal Society of Arts for 1920, has been awarded to Albert Abraham Michelson, professor of physics in the University of Chicago, whose optical inventions have ensured minute precision in measurements.